Examining mechanisms of self-control improvement

by

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I hereby declare that I am the sole author of this thesis. This is a true copy of the thesis, including any required final revisions, as accepted by my examiners.

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Abstract

Prior research provides evidence that people can improve their self-control performance through practice (e.g., Muraven, Baumeister, & Tice, 1999). Building on the Strength Model of self-control (Baumeister, Heatherton, & Tice, 1994; Muraven & Baumeister, 2000), this work assumes that self-control practice operates by increasing the capacity or endurance of a domain-general self-control resource. However, recent developments that highlight the role of motivation in self-control performance (e.g., Clarkson, Hirt, Jia, & Alexander, 2010; Job, Dweck, & Walton, 2010) suggest that changes in values, expectations, and beliefs may be driving the improvements over time. In the current study, I adapted a paradigm from the self-control training literature (Muraven, 2010a) in order to examine the possible role of motivational mechanisms in self-control performance improvement. Participants were randomly assigned to one of three practice conditions: a self-control task (avoiding sweets) or two control tasks. Self-control performance and potential motivational mechanisms were assessed both before and after the two-week practice period. Consistent with earlier research, self-control practice was associated with improved performance on an initial self-control performance task; however, there was no evidence of improvement in a post-depletion self-control task. Although self-control practice was not strongly associated with changes across potential motivational mechanisms, some exploratory analyses suggested that self-control instrumentality (beliefs that successful self-control is a means to central, self-relevant outcomes) may be an important predictor of self-control performance. I discuss implications for motivational models of self-control.

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Table of Contents

INTRODUCTION 1
The Strength Model of Self-Control 2
Evidence for a Resource-Based Depletion Effect 3
Evidence for a Resource-Based Practice Effect 4
Impetus for a Motivational Model of Self-Control 7
Evidence for a Motivation-Based Depletion Effect 7
Existing Evidence for a Motivation-Based Practice Effect 9
Proposed Mechanisms for a Motivation-Based Practice Effect 13
Self-Control Value 13
Expectations 14
Beliefs 15
OVERVIEW OF STUDY 16
METHOD 17
Participants and Design 17
Practice Instructions 17
In-Lab Assessments of Self-Control 19
Stroop Task 20
Depletion Manipulation 21
Anagram Task 21
Online Questionnaires 21
Self-Control Value 22
Self-Control Instrumentality 22
Self-Control Efficacy 23
Ideal Self and Ought Self 23
Working Memory 23
Chronic Individual Difference Measures (Time 1 Only) 24
Additional Time 2 Measures 24
RESULTS 26
Compliance 26
Primary Analyses 27
Baseline Measures of Self-Control (Time 1, Pre-Practice) 27
Stroop Performance 27
Anagram Persistence and Performance 27

Post-Practice Measures of Self-Control (Time 2, Post-Practice) 28
Stroop Performance 28
Anagram Persistence 29
Anagram Performance 29
Examining Possible Mechanisms of Self-Control Performance and Improvement 29
Baseline Questionnaire Measures (Time 1, Pre-Practice) 30
Post-Practice Questionnaire Measures (Time 2, Post-Practice) 30
General Self-Efficacy 30
Self-Control Value 30
Self-Control Instrumentality 31
Self-Control Efficacy 31
Limited Resource Beliefs 31
Trait Self-Control 32
Self-Esteem 32
What predicts baseline self-control performance? 32
Stroop Task 32
Anagram Persistence and Performance 32
DISCUSSION 33
Self-Control Performance 33
Interpreting the Stroop Effects 33
Interpreting the Anagram Effects 35
Potential Mechanism of Self-Control Improvement: Values, Expectations, and Beliefs 36
Changes Over Time 36
Mechansisms of Self-Control Improvment 38
Predicting Performance at Time 1 39
Implications and Future Directions 40
REFERENCES 43
Table 1 54
Table 2 56
Table 3 57
Table 4 58
Table 5 59
Table 6 60
Figure 1 61

Introduction

Self-control, or the effortful overriding of a dominant response, has been described as "one of the most powerful and beneficial adaptations of the human psyche" and "among the most widely studied constructs in the social sciences" (Tangney, Baumeister, & Boone, 2004, p. 272; Duckworth, 2011, p. 2639). The value of self-control has long been appreciated. One of the Founding Fathers of the United States advised: "Educate your children to self-control, to the habit of holding passion and prejudice and evil tendencies subject to an upright and reasoning will, and you have done much to abolish misery from their future and crimes from society" (Benjamin Franklin, 1706 - 1790). Indeed, beyond its significance for pursuing everyday goals, researchers posit that self-control is at the root of many serious societal problems, including alcoholism (Muraven, Collins, & Nienhaus, 2002), obesity (Elfhag & Morey, 2008), and violent crime (Denson, Capper, Oaten, Friese, & Schofield, 2011). One author goes so far to say there may be "no such thing as 'too much' self-control" (Duckworth, 2011, p. 2639). Whether or not self-control is invariably positive, a certain minimum level is no doubt essential to human functioning and well-being.

One striking example of the powerful influence of self-control is the longitudinal follow-up of participants in the "marshmallow" paradigm developed by Mischel and colleagues. In this line of study, pre-schoolers were given a choice between an immediate, smaller reward (e.g., one marshmallow) and a delayed, larger reward (e.g., two marshmallows) (Mischel, Ebbessen, & Zeiss, 1972). Cognitive tests, parental reports, and teacher reports a decade later found that what at first blush might seem a trivial behavior—number of seconds preschoolers could wait for the larger reward (delay of gratification)—significantly predicted academic and social outcomes, such as verbal fluency, ability to cope with stress, and social

adjustment of these adolescents (Mischel, Shoda, & Peake, 1988; Shoda & Mischel, 1990). Public policy research by Moffitt and colleagues (2011) provided further support for these findings with two large-scale longitudinal studies. In these samples, self-control performance in childhood was significantly related to physical health, substance dependence, personal finances, and criminal offending outcomes more than two decades later. Benjamin Franklin's advice, it seems, rings true.

Research not only supports the notion that improving self-control is *desirable*, but equally importantly, that self-control performance *can* be improved (e.g., Muraven, Baumeister, & Tice, 1999; Baumeister, Gailliot, DeWall, & Oaten, 2006; Oaten & Cheng, 2006a). The current study examines possible mechanisms that may underlie this self-control performance improvement. I begin by describing evidence that self-control performance can be improved through self-control training exercises, in the context of two competing theories of how self-control works: a literal strength (or resource-based) perspective and a motivational perspective.

The Strength Model of Self-Control

The strength model, an influential model in the self-control literature, also known as ego depletion theory (Baumeister, Heatherton, & Tice, 1994; Baumeister & Heatherton, 1996; Muraven & Baumeister, 2000), proposes that exerting self-control draws on a limited, domain-general resource. Consistent with many approaches, self-control is defined as the human mental capacity to inhibit immediate thoughts, temptations, and impulses in favor of more global or long-term goals; hence, self-control is one *type* of self-regulation (Fujita, 2011), whereby individuals manage their behaviors according to goals and perceived social standards.

The strength model makes three major claims about the way that self-control operates, given the assumption that self-control consumes a limited, domain-general resource (Baumeister, Bratslavsky, Muraven, & Tice, 1998; Baumeister, Muraven, & Tice, 2000). First, the theory states that exerting self-control in one domain (e.g., suppressing an emotion) depletes the resource and makes individuals more likely to fail in subsequent self-control efforts—even in seemingly unrelated domains (e.g., resisting sweets). Second, the theory posits that the only way to restore the depleted resource in the short-term is through rest or physiological replenishment (e.g., ingesting glucose, Baumeister, Vohs, & Tice, 2007). Third, in the long-term, by practicing self-control over a period of time, the pool of resources can be expanded, allowing individuals to improve self-control performance, much like building a muscle.

Evidence for a resource-based depletion effect. The bulk of the research in the strength model tradition employs a two-task paradigm to test the core assumption that self-control consumes or depletes a resource. In this paradigm, participants are first randomly assigned to a depleting (effortful overriding of an impulse) or non-depleting initial task; performance on a second self-control task, then, is the dependent measure. For example, in one of the earliest studies (Baumeister, Bratslavasky, Muraven, & Tice, 1998), participants watched a sad movie clip from *Terms of Endearment* with instructions to either suppress their emotions (depletion condition) or to experience and express emotions naturally (non-depletion control), and subsequently, performed a mental problem-solving task. Participants who suppressed their emotions were less likely to persist in the problem-solving task.

There are now hundreds of conceptual replications of this resource depletion effect: exhibiting self-control on an initial task depletes performance on a second task (for a meta-

analysis, see Hagger, Wood, Stiff, & Chatzisarantis, 2010). As might be expected from the very general terms of "overriding an impulse," depletion manipulations have employed a wide range of behaviors, including presenting oneself to a skeptical audience (Vohs, Baumeister, & Ciarocco, 2005), ostensibly "taste-testing" radishes instead of eating nearby chocolates (Baumeister et al., 1998), and making a difficult self-relevant decision (Baumeister, Sparks, Stillman, & Vohs, 2008). Some of the most common dependent measures have included persisting on an anagram (i.e. letter-scrambling) task when the opportunity to leave is available (Muraven, Shmueli, & Burkley, 2006), overriding the dominant response to process words in the Stroop task (Muraven et al., 2006), doing mental arithmetic (Vohs et al., 2005), and holding a spring-loaded handgrip until exhaustion (Muraven et al., 1998).

Overall, there is strong support for the idea that self-control performance declines after initial exertion (Hagger et al., 2010). Furthermore, it is commonly assumed that these performance effects offer clear support for the strength model. As I discuss below, more recent work suggests the need to revisit the conclusion that self-control performance declines require the assumption of a depleted resource (e.g., Muraven, 1998, Tyler & Burns, 2009; Baumeister, Muraven, & Tice, 2000, Job, Dweck, & Walton, 2010).

Evidence for a resource-based practice effect. While a large body of research has examined the depletion aspect of the strength model, relatively few studies have examined the second implication that practice can expand the resource. To my knowledge, only 14 studies have examined self-control improvement (Baumeister et al., 2006): undergoing just two weeks of self-control training has been linked to improved performance in laboratory tasks, including the Stop-Signal Task (Muraven, 2010a; Muraven, 2010b), which involves inhibiting a learned key-press response when a tone is played, and the Visual Tracking Task (e.g., Oaten & Cheng,

2006a), which involves focusing on three target circles while ignoring distractor stimuli. These two tasks are considered relatively raw measures of the one's cognitive ability to inhibit a dominant response, which according to the strength model should be at the root of self-control ability in all domains. Indeed, relating more directly to real-world experience, similar two-week training exercises have been linked to improved regulation of aggressive responses (Denson et al., 2011), reduced intimate partner violence (Finkel, DeWall, Slotter, Oaten, & Foshee, 2009), increased smoking cessation (Muraven, 2010b), and better control of impulse buying (Sultan, Joireman, & Sprott, 2012). Similar effects have been shown in the domains of exercise, academic study, and financial planning with self-control practice periods of two months (Oaten & Cheng, 2006a; Oaten & Cheng, 2006b; Oaten & Cheng, 2007).

The most commonly used paradigm includes a baseline measure of self-control, two weeks of self-control practice with monitoring, and a follow-up measure of self-control to assess changes from baseline (e.g., Muraven, 2010a). Because practicing self-control is assumed to have a direct effect on self-control performance by strengthening or expanding the resource, what is essential, according to the strength model, is that the practice task requires overriding an impulse or dominant response (Baumeister et al., 2007). Consequently, practice tasks have taken the form of avoiding sweets (Muraven, 2010a), keeping good posture (Sultan et al. 2012), using one's non-dominant hand for various tasks (Denson et al., 2011), and using proper speech (Gailliot, Plant, Butz, & Baumeister, 2007a).

Although many studies have not assigned participants to a control practice task (likely because effective control tasks are difficult to design), some studies have included control tasks to rule out the possibilities that self-control practice tasks have their effects simply because they are highly-involving, because of the difficulty of the practice tasks, or because they make

self-control highly salient. To address the argument that participants in the experimental conditions feel relatively more involved and thus place greater value on assigned laboratory tasks, Gailliot and colleagues (2007a) assessed post-practice self-regulatory success through daily life measures (e.g., eating behavior, studying), as opposed to having a follow-up lab session, and replicated the typical self-control improvement effect. Studies that have included practice tasks designed to control for task difficulty (Hui et al., 2009; Muraven, 2010a; Muraven, 2010b) provide some evidence that difficult tasks that do not require self-control are not enough to yield self-control improvement, though whether difficulty has truly been equated in these studies is not clear. Similarly, studies that have ensured that all participants are aware of the salience of self-control (and of the potential of the study to improve their self-control) have also found that the experimental self-control practice condition is uniquely linked to self-control improvement (Muraven et al., 1999; Muraven, 2010a; Muraven, 2010b). However, I would argue that the disconfirmation of these latter two alternative explanations (task difficulty and self-control salience) is still relatively ambiguous, as I will address in more detail in the discussion of possible motivational mechanisms.

Within the context of the strength model, researchers have discussed two possible ways that a self-control resource, like a muscle, could "expand" via practice: via an increase in power or endurance. To assess these theoretically distinct mechanisms, several studies (Muraven et al., 1999; Gailliot et al., 2007a; Oaten & Cheng, 2006a; Oaten & Cheng, 2006b; Oaten & Cheng, 2007) have included two measures of self-control in the baseline and post-practice lab sessions, one assessment prior to a depletion manipulation (an assessment of resource power or capacity) and one assessment post-depletion (an assessment of self-control resource endurance). In the study by Muraven and colleagues (1999), for example, each lab

session involved an initial test of holding a spring-loaded handgrip, a task of recording thoughts while specifically inhibiting thoughts of a white bear (a common depletion manipulation), and a second test of holding a spring-loaded handgrip. Thus, there were two ways to improve over the practice period: better initial performance (or power), as assessed the first handgrip test, and better sustained performance (or endurance), as assessed by the second. In this example, the authors observed an increase in endurance only, suggesting that self-control practice may work primarily by increasing endurance rather than capacity or power. However, many demonstrations since (Muraven, 2010a; Muraven, 2010b; Denson et al., 2011; Sultan et al., 2012) have revealed improvements using only one measure of self-control performance (without depletion); thus, research provides evidence that self-control practice may lead to improvements in *either* self-control power or capacity.

Impetus for a Motivational Model of Self-Control

While the strength model and its elegant analogy to a muscle have advanced the field immensely, a growing body of research provides evidence that self-control may not operate in the ways originally posited by the theory (e.g., Beedie & Lane, 2010; Job et al., 2010; Clarkson, Hirt, Jia, & Alexander, 2010); rather, motivational factors appear to play a more central role. In this section I describe motivational factors that have been identified as playing a role in both depletion (self-control decrement) and practice (self-control improvement) effects.

Evidence for a motivation-based depletion effect. A number of studies provide evidence that manipulating the value of the self-control tasks (e.g., by suggesting that performance will help others or will yield additional monetary remuneration) attenuates the typical depletion effect (e.g., Muraven & Slessareva, 2003; Muraven, 1998; Boucher & Helen, 2011). Other subtle interventions have been successful as well: self-affirmation (reflecting on a

personal value) (Schmeichel & Vohs, 2009), perceiving the exertion as a personal choice (Muraven, Gagné, & Rosman, 2008; Moller, Deci, & Ryan, 2006), and increased self-awareness (Alberts, Martijn, & DeVries 2011) all attenuate the depletion effect. These findings suggest that in at least some situations, performance depends more on commitment, rather than ability, to exert self-control.

Second, a number of studies provide evidence that perceptions and beliefs play a powerful role in determining depletion effects. Being told explicitly that performance on a first self-control task should not hinder subsequent performance (and may even be *re-energizing*) eliminates typical depletion effects (Martijn, Tenbult, Merckelbach, Dreezens, & DeVries, 2002). More recently, Job and colleagues (2010) found that people who believe that self-control is an unlimited resource do not demonstrate depletion effects, both when implicit beliefs were measured and manipulated. Likewise, perceiving your own resources as depleted (through misleading feedback) better predicts self-control than actual previous exertion (Clarkson et al., 2010). Participants have even been shown to be "depleted" and "replenished" vicariously (Ackerman, Goldstein, Shapiro, & Bargh, 2009; Egan, Hirt, & Karpen, 2012): for example, when participants read about a depleting experience of someone with whom they closely identify (e.g., another student; not a professor) they assimilated by subsequently performing worse in a self-control task themselves.

Further support for the role of motivation comes from recent evidence that glucose is not a probable physiological mechanism for depletion effects (Beedie & Lane, 2012; Kurzban, 2010; Molden et al., 2010). Although earlier work suggested that glucose may be the literal self-control resource (Baumeister et al., 2007; Gailliot et al., 2007b; Dvorak & Simons, 2009), mounting evidence suggests that glucose levels are not actually depleted during the typical

self-control exertion task (Kurzban, 2010; Khan, Vasquez, Gray, Wians, & Kroll, 2006; Molden et al., 2012) and that a glucose rinse can replenish energy without ingestion, suggesting that it may increase self-regulation via motivational, not energetic mechanisms (Molden et al., 2012). In short, the likeliest candidate for the resource itself turns out not to work as directly as once believed.

Taken as a whole, these findings imply that self-control depletion effects may be better explained by motivational factors than by the literal depletion of a single resource pool. Several alternative models (Kurzban, 2010; Kruglanski et al., 2012; Inzlicht & Schmeichel, 2012) have now emerged, highlighting the importance of resource *perceptions* rather than the hard limits of the resource itself. Across these models, two common themes or factors can be identified, related to expectations of self-control performance (e.g., "Will I be able to exert self-control?" "Will exerting self-control yield the outcomes I expect?") and value (e.g., "How desirable are these outcomes to me?"). These factors not only reflect the accumulating evidence in the strength model literature that factors such as incentives and implicit beliefs matter, but also capture two core facets of classic motivational theories (Vroom, 1964; Atkinson & Feather, 1966).

Existing evidence for a motivation-based practice effect. Relatively few studies have attempted to systematically examine motivational mechanisms for self-control improvement through practice. Though intuitively feasible that a literal resource or physiological capacity is directly strengthened or expanded, the strength model does not offer many ways of measuring this resource to test the proposed mechanism, apart from observing changes in self-control performance. However, a few studies have examined the possibility of an indirect pathway,

assessing potential alternative mechanisms (Oaten & Cheng, 2006a; Oaten & Cheng, 2006b; Oaten & Cheng, 2007; Muraven, 2010a; Muraven, 2010b; Hui et al., 2009).

None of the existing studies provide strong support for an indirect effect of self-control training, although the examinations have been relatively sparse. Oaten and Cheng (2006a, 2006b, 2007) examined self-reported general self-efficacy, stress, and emotional distress as potential mediators of the self-control practice effect. Although perceived stress and emotional distress were reduced during the self-control training regimen of physical exercise (Oaten & Cheng, 2006b), these factors did not mediate the improvements in self-control performance of participants in the experimental groups relative to control, as assessed by a visual tracking lab task and daily life indicators (e.g., doing household chores, keeping commitments). Similarly, self-efficacy and stress did not mediate improvements demonstrated in training programs of academic study and financial monitoring (Oaten & Cheng, 2006a; Oaten & Cheng, 2007). Importantly, however, research on self-efficacy suggests that perceived self-efficacy does not necessarily generalize from one situation to another, unless the situations are perceived as drawing on the same personal attributes (Cervone, 2000). Therefore, the general measure of self-efficacy that was employed in these studies may not be as sensitive to confidence about one's ability to exert self-control specifically; it may be premature to rule out self-efficacy as a potential moderator. Furthermore, although Oaten & Cheng (2006a, 2006b, 2007) found no evidence that self-efficacy and stress assessed post-practice mediated self-control performance improvement, these analyses did not examine whether *changes* in self-efficacy or stress levels were related to self-control improvement.

Muraven (2010a, 2010b) also provided some examination of potential motivational mechanisms through practice reports that participants completed on a daily basis. In a lab-

based study (Muraven, 2010a) and a smoking-cessation study (Muraven, 2010b) participants were assigned to one of four training tasks: two experimental self-control practice tasks that involved overriding impulses (avoiding sweets and holding a handgrip) and two control tasks that did not involve self-control (doing daily math problems and keeping a self-control diary). Over the two weeks of training, participants reported each day on their perceptions of practice tasks: specifically, how much they practiced, how much effort they felt they exerted, whether the assigned task required self-control, and whether they believed practicing would help build self-control. Participants in the control conditions were equally likely to believe practicing would help build their self-control, and yet they did not exhibit improved performance on a Stop-signal task (Muraven, 2010a) or in the number of days they successfully refrained from smoking (Muraven, 2010b). The conclusion, consistent with the strength model, was that the practice of effortful overriding itself is the "active ingredient" in building self-control, not the value of self-control or expectations for self-control improvement.

Although these conclusions may suggest that there is no need to further examine intervening mechanisms, a closer look at the pattern of results suggests some important hints about intervening processes. First, the math and self-control diary conditions, designed to control for difficulty and the salience of self-control, may not have been equally effective as controls (e.g., participants in the math condition did not perceive that they exerted self-control to the same extent), a point I will return to in the discussion. Interestingly, Muraven (2010a, 2010b) did find that, only for participants in the two experimental conditions, the average perceived effort was related to improved inhibition in the Stop-signal task at Time 2. However, Muraven (2010a) did not report whether *changes* in these daily measures were related to performance in the Stop-signal task at Time 2. This is another viable way to test for possible

mechanisms, as I adopted in my own approach. Additionally, in the smoking study (2010b), he found that participants were able to quit smoking longer to the extent that they felt that they exerted self-control on their practice task. This effect was strongest for participants in the experimental conditions. Thus, Muraven's work provides some indication that expectations matter, though it is unclear whether these expectations must arise from a particular type of self-control practice task. Furthermore, his designs did not permit a systematic examination of how the trajectory of such beliefs (e.g., believing you are exerting self-control, believing you will improve) may be related to changes in self-control performance.

Lastly, Hui and colleagues (2009) provide some additional evidence that suggests the importance of considering motivational mechanisms of self-control performance. Participants in this study were assigned to a two-week training program in which the level of self-control practice demands were manipulated. In the high-demand practice (strong training) condition, participants had to complete difficult Stroop tasks and rinse with an unpleasant mouthwash every day. In the low-demand practice (weak training) condition, participants completed easy Stroop tasks and rinsed with a mild mouthwash every day. A third group of participants completed no practice tasks. The primary dependent measures were standard self-control tasks of mental concentration and cold tolerance; critically, cardiovascular measures were also used during these tasks to assess effort-related responses. They found that even participants in the weak training condition improved significantly in self-control performance, though less so than those in the strong training condition. Furthermore, of particular interest for the current study, the cardiovascular measures revealed that participants in the strong training condition actually had the highest effort-related responses in both post-practice self-control tasks. This suggests that practice may not have made the self-control tasks easier, but may have changed

perceptions of their attainability or value, such that participants in the strong-training condition were more willing to exert effort. Motivation and engagement, this suggests, are critical to improved self-control performance.

Given the fairly limited and mixed evidence regarding possible mechanisms of self-control improvement (e.g., tolerance of stress, value, expectations of improvement) and the number of candidates still entirely unexplored (e.g., self-control-specific self-efficacy, self-control's perceived personal relevance), I believe it would be premature to conclude that there are no viable mechanisms for self-control practice effects. And indeed, the growing body of work on the role of motivation in self-control depletion effects suggests that the notion of a literal self-control resource needs to be revisited.

Proposed Mechanisms for a Motivation-Based Practice Effect

Possible mechanisms must be variables that can both plausibly influence self-control performance and could reasonably be expected to change over two weeks. Building on the factors identified by recent advances as moderators of the depletion effect (e.g., values, expectations, beliefs; Muraven & Slessareva, 2003; Martijn et al., 2002; Clarkson et al., 2010; Job et al., 2010), I propose that these factors likely play a critical role in self-control improvement.

Self-Control Value. One candidate mechanism involves how self-control practice may change people's experience of the value of self-control in their lives. Recall that depletion effects are attenuated simply by reflecting on personal values, by feeling you are choosing to exert yourself, and by believing that exertion will pay off for yourself or others (Muraven & Slessareva, 2003; Muraven, 1998; Boucher & Helen, 2011; Schmeichel & Vohs, 2009; Muraven et al., 2008; Moller et al., 2006; Alberts et al., 2011). It appears that self-control

success is largely dependent on self-motivation. This general question can be thought of in two ways: "How desirable is self-control generally?" which I will refer to as self-control value, and "How relevant are self-control outcomes to important aspects of self?" which can be characterized as self-control instrumentality, the extent to which self-control is seen as a means to important ends (Vroom, 1964). These candidates also meet the criteria of flexibility: we might reasonably expect that after incorporating a new task into their daily lives, participants have an opportunity to see how such simple adjustments are possible and worthwhile (as Hui et al., 2009 suggest), thus motivating them to increase exertion.

Expectations. A related question is how confident participants are that they can effectively exert self-control. Recall that depletion effects are attenuated by being told that an initial task of overriding an impulse will not be depleting or by simply *perceiving* your own resources as high (Martijn et al., 2002; Ackerman et al., 2009; Egan et al., 2012). It appears that self-control success is also very dependent on one's perception of one's own abilities. Though general self-efficacy has not been shown to mediate prior self-control practice effects (Oaten & Cheng, 2006a; Oaten & Cheng, 2006b; Oaten & Cheng, 2007), self-efficacy beliefs specific to self-control have not been assessed as closely. Even Muraven's closer examination (2010a, 2010b), with the daily questionnaire item "Practicing will help build my self-control," yielded somewhat mixed results. Furthermore, this item perhaps most directly assessed the extent to which participants believed *practice* was effective, rather than directly assessing self-control self-efficacy beliefs per se. Thus, it is important to more systematically and directly examine if self-control self-efficacy ("How much power do I have to exert self-control effectively?") changes through self-control practice. Again, practicing a self-control task may reasonably be expected to improve this kind of confidence over a short period of time,

provided that participants are successful in their assigned task. This question can be seen as a complement to value in that participants must both believe they can attain self-control outcomes and believe that self-control outcomes are valuable to truly be motivated.

Beliefs. A final question is whether participants believe that exerting mental effort and resisting temptation are exhausting in the first place. These implicit theories of self-control have been investigated in the past (Job et al., 2010), using established scales to assess the degree to which participants endorse a limited resource theory (e.g., "resisting temptations makes you feel more vulnerable to the next temptations that come along"). As briefly discussed above, endorsing more unlimited theories of self-control predicts resistance to depletion effects (Job et al., 2010), both when these beliefs were measured and when manipulated. A longitudinal study demonstrated its generalizability, as well, which included predicting improved eating and study habits over multiple months, as well as progress toward a self-selected personal goal. As these theories about the nature of self-control itself are flexible and have never been addressed directly in the context of self-control practice effects, they are also a very plausible candidate mechanism of self-control improvement.

Overview of Study

In this study, I examined potential mechanisms of the self-control practice improvement effect observed in a number of earlier studies (e.g., Oaten & Cheng, 2006, Muraven, 2010a). The study followed the typical two-week practice paradigm, with an initial and post-practice lab session to assess changes in self-control performance. As in the original demonstration (Muraven et al., 1999), the current study included assessments of self-control performance both when participants were and were not depleted in order to observe changes in both self-control "capacity" and self-control "endurance." Critically, in addition to the lab sessions, participants completed a number of measures to assess potential mechanisms of self-control practice improvement both before and after the two-week training period.

I hypothesized (1) that performance in both pre- and post-depletion self-control tasks would improve over the two weeks of self-control practice (avoiding sweets) and that they would improve relative to those in control conditions (math; self-control diary). Most importantly, I hypothesized (2) that individual differences in values, expectations, and beliefs (e.g., self-control value, self-control instrumentality, self-control efficacy, implicit theories of self-control) would predict baseline self-control performance measures and (3) that changes in these variables would mediate improvement from baseline to post-practice measures.[1]

[1] In the current paper, for completeness, I include in the method a description of every measure included in the study. However, I will be focusing on a limited subset of analyses.

Method

Participants and Design

126 undergraduate students in psychology courses at the University of Waterloo were recruited to participate in this study for $20 and course credit. Additionally, participants received a ticket for a $100 lottery for each of the 14 daily online questionnaires they completed. Out of the initial 126 participants, 9 participants did the online questionnaire but never made it to the first session and 4 were cancelled by the first lab session due to external circumstances (e.g., technical error, snow day). Only 4 participants dropped out after completing the initial lab session (i.e., during the practice period), leaving 109 participants (87 females and 22 males) who completed the entire study. They were a majority White (43%) and Asian (41%) students, with a mean age of 19.96 years (*SD* = 1.70).

Participants completed an initial online questionnaire, a laboratory session 3-7 days later (*M* = 4.68, *SD* = 2.19) that involved assessment of baseline self-control performance, 14 days of a self-control practice task, a second online questionnaire 1-3 days later (*M* = 1.56, *SD* = 0.88), and a final laboratory assessment of self-control 1-3 days after (*M* = 1.36, *SD* = 0.96) the second online questionnaire. Participants were randomly assigned to one of three practice conditions: one experimental condition which did involve inhibiting a dominant response (avoiding sweets) and two intended as control conditions (keeping a diary of self-control efforts, doing daily math problems) based on conditions used in previous research (e.g., Muraven, 2010a; Muraven, 2010b).

Practice Instructions

To replicate previous demonstrations of the effects of self-control practice (Muraven, 2010a; Muraven, 2010b), the materials provided for all practice conditions led participants to

believe that practicing their assigned task regularly should build their self-control capacity. However, according to previous research in this tradition (Baumeister et al., 1998), only one of the practice conditions, avoiding sweets, actually involved effortful inhibition of an impulse. Thus, according to the strength model, only this condition should lead to improvements in self-control (Muraven, 2010a).

In the avoiding sweets condition, participants were asked to refrain from eating dessert foods such as cakes, candies, pies, cookies, and any other sweet treats as much as possible in the following 2 weeks. Two participants who said they rarely eat sweets were re-assigned to a different condition.

In the self-control diary condition, participants received a diary packet for both weeks and were asked to log every time they exerted self-control, returning the packet at the second lab session. Each entry (up to 12 per day plus extras in the back) had space for the time, the temptation, the goal with which it conflicted, any strategies used to resist, comments, and four 7-point scales that were carefully explained: strength of the temptation, strength of the goal, how much work went into resistance, and how successful the resistance was. A variety of examples were given in the instruction sheet and in a diary sample page, such as resisting the urge to eat unhealthy food and wanting to scream in anger but maintaining composure, to illustrate my definition of self-control as well as how to use the scales properly.[2] As in Muraven's (2010a, 2010b) studies, this condition was designed to rule out the possibility that any changes observed in the avoiding sweets condition could be due simply to the salience of self-control.

[2] Future planned analyses will permit examination of the relationship between diary responses and key dependent variables.

In the math condition, participants worked on a set of simple arithmetic problems for 2 minutes twice a day, in a packet provided to them and returned at the end of the practice period. In Muraven's studies (2010a, 2010b), this condition was intended to be a task of approximately equal difficulty to avoiding sweets, to rule out the alternative explanation that practicing any demanding task could improve self-control. Although I questioned the assumption that these two tasks were equivalent in difficulty, I believed it was important to try to replicate faithfully the earlier studies; further, regardless of difficulty, it controlled for sense of involvement in the study (Gailliot et al., 2007a, Study 3).

Additionally, as in Muraven's studies (2010a, 2010b), participants in all conditions were asked to complete a brief practice report every day to assess compliance and perceptions of the study. In the current study, I included 2 items asking whether or not they practiced (yes/no) and how many times; 1 item asking how much they were aware or thought about the task on a 5-point scale from 1 (Not at all) to 5 (All day); and, 3 items stating that practicing was "effortful," "fun," and "successful" to which they indicated their agreement, if applicable, on a 5-point scale from 1 (Strongly Disagree) to 5 (Strongly Agree). It was emphasized that regardless of actual persistence or success, what was most important was that they complete the practice reports honestly. To encourage compliance, I also gave participants a ticket in a lottery to win $100 at the end of the term for every practice report they filled out during the 2 weeks, regardless of whether they reported complying or not. A research assistant monitored the completion of practice reports and e-mailed reminders for any missing days to maximize feedback.

In-Lab Assessments of Self-Control

The lab sessions involved a direct measure of self-control, a depletion manipulation, and a second measure of self-control (Muraven et al., 1999; Gailliot et al., 2007a). In the initial lab session, the three tasks were followed by the practice instructions, whereas in the final lab session, they were followed by payment and debriefing. The procedures in each session were identical although the specific materials (e.g., letters for the anagram task) were different across the two sessions.

Stroop task. Participants first completed the Stroop task (Stroop, 1935), a task which has frequently been used as a measure of self-control (Job et al., 2010; Webb & Sheeran, 2003).

Participants were presented with one of four color words on a screen. On "congruent" trials, the ink color matched the text (e.g., the word "red" displayed in red font). On "incongruent" trails, the ink color did not correspond to the text (e.g., the word "red" displayed in blue font). The letters "R," "T," "Y," and "U" on the keyboard were labelled with a red, blue, yellow, and green sticker, respectively, and the task of participants was to respond by pressing the key for the color in which the word was written as quickly and accurately as possible. Thus, on incongruent trials, participants must override the dominant response to read the color word. After 3 examples and 12 practice trials, participants did 97 experimental trials.

Stroop interference scores were calculated by a standard procedure (Salo, Henik, & Robertson, 2001): reaction times on correctly-responded trials were log-transformed (skew > 1 for T1 and T2 reaction times) and the average log-transformed reaction times on congruent trials for each participant were subtracted from the average log-transformed reaction times on incongruent trials. Thus, higher interference scores represent more delay in responding on

incongruent trials relative to congruent trials, or, greater difficulty inhibiting the dominant response of reading.

Depletion manipulation. Following the Stroop task, participants completed a difficult version of the e-crossing task, a commonly used depletion manipulation (e.g., Molden et al., 2012; Baumeister et al., 1998). Participants were first given a sheet of typewritten text and asked to cross out each instance of the letter *e*. After learning this rule, however, they were given a second sheet of typewritten text and asked to follow a more complex rule of crossing out every *e,* unless another vowel was found two letters preceding it or the next letter after. Thus, participants had to follow a complex new rule and inhibit the rule they had previously used.

Anagram task. The second self-control measure was a multiple solution anagram task, another commonly used measure of self-control. Participants were given 7 letters ("U, R, A, E, O, C, G" at Time 1 and "C, L, A, T, P, S, I" at Time 2) and had as long as they wanted to come up with as many solutions as possible. Thus, participants have to choose to persist, resisting the urge to quit (Baumeister et al., 1998).[3] Both the length of time participants persisted (Muraven et al., 2006; Tyler & Burns, 2009) and the number of solutions (Gailliot et al., 2007a) were recorded.

Online Questionnaires

At both time-points participants completed 13 scales (the possible motivational mechanisms) as well as a working memory task. Table 1 lists each scale as well as its source, a sample item or description, and scale anchors. All mechanism scales were presented in random

[3] Different specific letters were chosen for the anagram task at Time 1 and Time 2 to avoid practice effects. An informal pilot test had suggested that the letter sets were equivalent in difficulty; however, the results suggest that the Time 2 anagram task may have been easier. I am currently conducting a formal pilot test to examine if the anagram tasks differ in difficulty. Analyses that examine changes from Time 1 and Time 2 use Z-scores to control for this potential confound.

order. At the initial online session participants also completed a number of individual difference measures, detailed below. At the post-practice online session participants completed some additional measures related to their experience with the practice task and their expectations for the upcoming lab session.

I included 8 established scales: The New General Self-Efficacy Scale (Chen, Gully, & Eden, 2001), Limited Resource Beliefs (Job et al., 2010), Locus of Control (Craig, Franklin, & Andrews, 1984), Trait Self-Control (Tangney et al., 2004), Cognitive Appraisal (Skinner & Brewer, 2002), Implicit Theory of Personality (Dweck, 1999), Implicit Theory of Intelligence (Hong, Chiu, Dweck, Lin, & Wan, 1999), and Self-Esteem (Robins, Hendin, & Trzensniewski, 2001). See Table 1 for additional information.

Self-Control Value. To assess the extent to which participants found self-control valuable, I created a six-item Self-Control Value Scale ($\alpha = .814$). Participants indicated their agreement on a 6-point scale from 1 (disagree) to 6 (agree) with the following items: "Exerting self-control is rewarding," "Exerting self-control pays off," "Self-control isn't that important to me" (reverse-scored), "I want to be able to exert self-control effectively," "I'd like to improve my self-control," and "It is important to me to do my best at exerting self-control."

Self-Control Instrumentality. To assess the degree to which participants view self-control as a means to success in self-relevant domains, I created a 7-item Self-Control Instrumentality scale ($\alpha = .897$) in which participants indicated how self-control success would improve several important aspects of their life, drawing on seven of Tomaka and colleagues' (1999) aspects of the self (overall physical well-being, the well-being of my loved ones, self-esteem, reputation with friends, reputation with family, likelihood of reaching important personal goals, performance at school). Participants indicated their agreement with one item

for each domain (e.g., Being successful at self-control will improve…my overall physical well-being") on a six-point scale ranging from 1 (strongly disagree) to 6 (strongly agree).

Self-Control Efficacy. To assess perceived self-efficacy specifically in the domain of self-control, I created a 15-item Self-Control Efficacy Scale ($\alpha = .946$). I adapted Manstead and Eekelen's (1998) perceived behavioral control phrases ("Whether or not I can… is completely up to me," "There's a lot I can do to be sure that I…," and "How confident are you that you can…?") to various ways of thinking about self-control: "continue to perform mentally strenuous activities," "override impulses," "resist temptations," "stay focused on a goal over time," and "concentrate mental energy on effortful tasks." Participants indicated their agreement on a 6-point scale from 1 (disagree) to 6 (agree) for items in the first two formats and on a 6-point scale from 1 (very doubtful) to 7 (very confident) for the last.

Ideal Self and Ought Self. Finally, to assess the extent to which participants felt that they were close to their "ideal self" and "ought self," I adapted Aron, Aron, & Smollan's Inclusion of Other in Self (1992) scale. The Inclusion of Other is Self scale is one item which portrays 7 images of two increasingly overlapping circles, labeled as "Self" and, in this case "Ideal Self"—with the instructions describing this as the type of person you hope, wish, or aspire to be. In the case of "Ought Self," the instructions describe this as the type of person it is your duty, obligation, or responsibility to be. Participants chose the image that represented their current relationship with their ideal/ought selves. This could be seen as a complement to the Regulatory Focus scale as it taps into progress towards promotion-focused goals and prevention-focused goals respectively.

Working Memory. After the scales relevant to potential mechanisms, both online questionnaires were concluded by a task of Working Memory Capacity (Oberauer, Sub,

Schulze, Wilhelm, & Wittmann, 2000). Working memory has frequently been linked to self-regulation (e.g., Hofmann, Gschwender, Friese, Wiers, & Schmitt, 2008, Kane & Engle, 2003), e.g., as a mediator of the effects of glucose on self-control performance (Denson et al., 2011) and thus, was included for exploratory purposes as a potential mechanism. This particular task (from Hofmann et al., 2008) involves indicating whether a series of simple mathematical equations (e.g. 3 + 5 = 7) are "true" or "false," while memorizing their one-digit results, and at the end of a series, reciting them. Through the ten trials, the number of equations in the series increases from four to eight.

Chronic Individual Difference Measures (Time 1 only). In the baseline questionnaire participants also completed the Regulatory Mode scale (Kruglanski et al., 2000), the Regulatory Focus Questionnaire (Higgins et al., 2001) and its composite (Haws, Dholakia, & Bearden, 2009), the Big Five Inventory (John & Srivastava, 1991), and the Behavior Identification Form (Vallacher & Wegner, 1989). Additional information about these scales is provided in Table 1.

Additional Time 2 Measures. In the post-practice questionnaire only, I included 19 items asking participants about their experiences during the practice period and 14 items about their expectations for the upcoming lab session. All items about the overall practice period experience were on a 7-point scale ranging from 1 (Strongly Disagree) to 7 (Strongly Agree): 6 items related to perceived difficulty and demand, similar to Muraven's (2010a, 2010b) daily questionnaire items, e.g., "Practicing was effortful," "Practicing required overriding an impulse", 8 items related to perceived value and instrumentality, e.g., "I was really invested in doing the practice tasks," "Practicing got me energized to work hard on other demanding

tasks", and 5 items related to personal performance, including general items, e.g., "I did well at the practice tasks", and trajectory-related items, e.g., "I got worse at the practice tasks".

Fourteen items about the upcoming lab session were also on a 7-point scale ranging from 1 (Strongly Disagree) to 7 (Strongly Agree). For each task (Stroop, Anagram), participants completed items related to task value and self-efficacy. For each task, 4 items were related to task value (e.g., "It is important to me to do my best on this task") and 3 items were related to task self-efficacy or expectations (e.g., "I am uncertain that I can respond quickly and accurately").

Results

Compliance

There are two methods we can use to assess participant compliance—the number of daily reports completed and the percentage of participants who indicated in their daily reports that they did their assigned practice task. Of the 109 participants who completed the study, 89% sent in a daily practice report for at least 8 days of the practice period. The mean reporting-compliance was 12.06 days, and median and mode were both 14 days. There was a marginal main effect of condition on compliance, F (2, 105) = 2.840, p = .063.[4] Tukey post-hoc comparisons of the three groups indicate that participants in the math condition (M = 11.06, SD = 3.90) completed fewer daily reports than those in the sweets condition (M = 12.89, SD = 2.19), p = .049. No other contrasts were significant: participants in the diary condition (M = 11.92, SD = 3.46) were statistically equivalent to those in the sweets, p = .416, and math conditions, p = .498.

Of the 109 participants, 80.7% indicated that they did their assigned practice task for 8 or more days (M = 11.09 days, Mdn = 13, $Mode$ = 14). There was a significant effect of condition on the number of days of practice, F (2, 106) = 4.593, p = .012.[5] The same pattern of contrasts emerged as for completing the daily reports: Tukey post-hoc comparisons indicated that participants in the math condition (M = 9.75, SD = 4.27) practiced fewer days than those in the sweets condition (M = 12.33, SD = 2.54), p = .009. No other contrasts were significant: participants in the diary condition (M = 11.19, SD = 3.84) were statistically equivalent to those in the sweets, p = .372, and math conditions, p = .211.

[4] Note that when using the exclusion of one univariate outlier on the Stroop measure, as in the remainder of analyses, this effect was only trending, F (2, 105) = 2.224, p = .113.

[5] Note that when using the exclusion of one univariate outlier on the Stroop measure, as in the remainder of analyses, this effect was still significant, F (2, 105) = 3.861, p = .024.

Primary Analyses

Self-control performance was assessed pre- and post- practice using a Stroop task (Stroop, 1935) and a multiple-solution anagram task. Data from the 109 participants who completed the study were screened for skew, kurtosis, and univariate outliers. At both sessions, all 3 lab measures were found to be normally distributed. Due to a technical error, there was missing data for one participant's initial Stroop score and another participant's post-practice Stroop score. Additionally, one participant was excluded based on a post-practice Stroop error rate of 17.9%, more than three standard deviations above the mean. This left 108 participants for analysis. For descriptive statistics, see Table 2.

Baseline Measures of Self-Control (Time 1, Pre-Practice).

Stroop Performance. As expected, prior to assignment to practice conditions, a one-way ANOVA on Stroop RT interference confirmed that there were no significant baseline differences by condition, F (2, 104) = 2.130, p = .124 and no contrasts were significant: sweets condition (M = 0.051, SD = 0.046), math condition (M = 0.075, 95%, SD = 0.056), and diary condition (M = 0.064, SD = 0.046).

Anagram Persistence and Performance. Also as expected, participants' baseline scores for anagram persistence (in seconds) did not differ by condition: F (2, 105) = 0.278, p = .758: sweets condition (M = 338.79, SD = 278.30), math condition (M = 294.17, SD = 222.44), diary condition (M = 323.63, SD = 263.26). Likewise, there were no significant condition differences for baseline anagram performance, in number of correct solutions: F (2, 105) = 1.039, p = .357: sweets condition (M = 12.72, SD = 6.20), math condition (M = 15.23, SD = 7.26), diary condition (M = 14.08, SD = 8.36).

Post-Practice Measures of Self-Control. See Table 3 for descriptive statistics for all Time 2 lab measures by condition.

Stroop Performance. A one-way ANOVA on Time 2 Stroop interference scores revealed a significant main effect of condition, F (2, 104) = 3.150, p = .047. Tukey post-hoc comparisons revealed that participants in the sweets condition (M = 0.039, SD = 0.037) had reduced interference (i.e., better performance) compared to the math condition (M = 0.061, SD = 0.034), p = .037, but did not differ from the diary condition (M = 0.048, SD = 0.048), p =.509. Likewise, the math and diary conditions did not significantly differ, p = .331.

When controlling for Time 1 Stroop scores, the main effect of condition became marginally significant, F (2, 102) = 2.216, p = .114. However, the pattern of contrasts remained the same. Participants in the sweets condition (M = 0.042, SD = 0.033) had reduced interference relative to the math condition (M = 0.058, SD = 0.033), p = .046, and were no different than those in the diary condition (M = 0.046, SD = 0.033), p = .597. Again, the math and diary conditions did not significantly differ, p = .129.

To directly examine the role of time, a repeated measures ANOVA was conducted. This analysis revealed a significant main effect of time, F (1, 103) = 11.028, p = .001, such that participants showed reduced interference on the Stroop task after the two-week practice period (M = 0.048, SD = 0.036) relative to baseline (M = 0.064, SD = 0.049). There was also a significant effect of condition averaging across both times, F (2, 103) = 3.492, p = .034: sweets condition (M = 0.046, SD = 0.031), math condition (M = 0.068, SD = 0.035), and diary condition (M = 0.055, SD = 0.035). However, despite the pattern observed above, there was no interaction between time and condition, F (2, 103) = 0.060, p = .942.

Anagram Persistence. Differences by condition in the anagram task were analyzed using standardized scores since a different set of letters was used for each session. For ease of interpretation, however, descriptives are provided in the original units (seconds). A one-way ANOVA examining Time 2 persistence was not significant, controlling for Time 1 persistence, F (2, 104) = 0.007, p = .993, or without controlling for Time 1 persistence, F (2, 105) = 0.159, p = .853 (sweets condition, M = 392.16, SD = 233.73, math condition, M = 359.47, SD = 268.49, diary condition, M = 380.69, SD = 239.53). A repeated measures ANOVA on the standardized scores revealed no effect of time, F (1, 105) < 0.001, p = .983, no effect of condition, F (2, 105) = 0.263, p = .769, and no interaction, F (1, 105) = 0.022, p = .979.

Anagram Performance. The results examining standardized anagram performance scores were parallel: a one-way ANOVA on the number of correct solutions at Time 2 was not significant, controlling for Time 1 persistence, F (2, 104) = 0.245, p = .783, or without controlling for Time 1 persistence, F (2, 105) = 0.500, p = .608: sweets condition (M = 24.81, SD = 11.45), math condition (M = 26.91, SD = 10.51), diary condition (M = 24.65, SD = 10.00). A repeated measure ANOVA on standardized scores revealed no effect of time, F (1, 105) = 0.027, p = .871, no effect of condition, F (2, 105) = 0.003, p = .953, and no interaction, F (2, 105) = 0.393, p = .676.

Examining Possible Mechanisms of Self-Control Performance and Improvement

As noted in the introduction, a subset of the scales included in the online questionnaires will be the focus of the current analyses: general self-efficacy, self-control value, self-control instrumentality, self-control self-efficacy, limited resource beliefs (both about the ability to continue mental activity and to continue resisting temptation), trait self-control ability, and self-esteem. In addition to the possible mechanisms highlighted in the introduction, I included

trait self-control and self-esteem in the analyses below, two relatively stable individual difference measures that have been strongly related to self-control performance in past research (Tangney et al., 2004; Judge, Bono, & Thoreson, 2002).

Baseline Questionnaire Measures. One-way ANOVAs on each of these measures at baseline confirmed that most measures were no different by condition; however, there was a failure of random assignment with regards to limited resources beliefs—specifically, there were differences by condition on implicit beliefs about the ability to continue resisting temptations: F (2, 105) = 5.779, p = .004. Tukey post-hoc comparisons revealed that participants in the sweets condition (M = 3.907, SD = 0.927) and diary condition (M = 4.108, SD = 0.916) had more unlimited theories (e.g., "If you have just resisted a temptation, you feel strengthened and can withstand any new temptations") than those in the math condition (M = 3.376, SD = 0.977), p = .050 and p = .004 respectively. Participants in the sweets and diary conditions did not differ from each other, p = .364. When controlling for initial implicit beliefs about temptation, the effect of time on Stroop interference was no longer significant, F (1, 102) = 0.580, p = .448, but critically, the effect of condition remained, F (2, 102) = 3.872, p = .024; there was no interaction, F (2, 102) = 0.001, p = .980.

Post-Practice Questionnaire Measures.[6]

General self-efficacy. A repeated-measures ANOVA on the New General Self-Efficacy Scale revealed no significant effect of time, F (1, 104) = 2.045, p = .156, or condition, F (2, 104) = 1.787, p = .173. There was no interaction, F (2, 104) = 0.052, p = .949.[7]

Self-control value. A repeated-measures ANOVA revealed marginally reduced endorsement of beliefs that self-control is valuable at Time 2, F (1, 104) = 3.884, p = .051

[6] One-way ANOVAs controlling for Time 1 scores show a similar pattern of results.
[7] When including only participants who practiced greater than 7 days, however, general self-efficacy did increase significantly, F (1, 85) = 4.717, p = .033 (M_{Time1} = 3.66, SD = 0.57; M_{Time2} = 3.75, SD = 0.56).

(M_{Time1} = 5.036, *SD* = 0.515; M_{Time2} = 4.93, *SD* = 0.59). This decrease did not differ by condition *F* (2, 104) = 1.698, *p* = .188, nor was there an interaction, *F* (2, 104) = 1.069, *p* = .347.

***Self-control instrumentality*.** A repeated-measures ANOVA revealed marginally reduced endorsement of beliefs in the instrumentality of self-control at Time 2, *F* (1, 104) = 3.428, *p* = .067 (M_{Time1} = 4.88, *SD* = 0.67; M_{Time2} = 4.74, *SD* = 0.72). This decrease did not differ by condition *F* (2, 104) = 0.464, *p* = .630, nor was there an interaction, *F* (2, 104) = 0.124, *p* = .883.

***Self-control efficacy*.** A repeated-measures ANOVA revealed no significant effect of time, *F* (1, 104) = 0.002, *p* = .966, or condition, *F* (2, 104) = 0.628, *p* = .536. There was no interaction, *F* (2, 104) = 0.605, *p* = .548.[8]

***Limited resource beliefs*.** A repeated-measures ANOVA revealed that belief in the ability to continue mental activity increased over time, *F* (1, 104) = 4.759, *p* = .031 (M_{Time1} = 2.72, *SD* = 0.86; M_{Time2} = 2.88, *SD* = 0.86), but did not differ by condition, *F* (2, 104) = 0.103, *p* = .902; nor was there an interaction, *F* (2, 104) = 0.255, *p* = .775. Belief in the ability to continue resisting temptation did not increase uniformly over time, *F* (1, 104) = 0.546, *p* = .462, but there was a main effect of condition, *F* (2, 104) = 3.620, *p* = .030, and a significant interaction, *F* (2, 104) = 4.425, *p* = .014, such that participants in the math condition had significantly more unlimited theories at Time 2, *F* (1, 33) = 6.125, *p* = .019, (*M* = 3.67, *SD* = 1.09); those in the sweets condition remained the same, *F* (1, 35) = 1.271, *p* = .267, (*M* = 4.08, *SD* = 0.85); and those in the diary condition had marginally reduced unlimited theories *F* (1, 36) = 3.339, *p* = .076, (*M* = 3.83, *SD* = 0.98). See Figure 1.

[8] For the subscale relating specifically to resisting temptation, however, (e.g., "Whether or not I can resist temptations is completely up to me"), there was a marginal interaction: *F* (2, 104) = 2.569, *p* = .081.

Trait self-control. A repeated-measures ANOVA on trait self-control revealed no significant effect of time, F (1, 104) = 0.381, p = .538, or condition, F (2, 104) = 1.167, p = .315. There was no interaction, F (2, 104) = 0.222, p = .801.

***Self-esteem*.** Finally, a repeated-measures ANOVA on self-esteem also revealed a marginal decrease over time, F (1, 104) = 3.371, p = .069 (M_{Time1} = 3.25, SD = 1.12; M_{Time2} = 3.17, SD = 1.09). This decrease did not differ by condition F (2, 104) = 0.252, p = .778, nor was there an interaction, F (2, 104) = 0.736, p = .481.

What predicts baseline self-control performance?

Given that relatively few post-practice condition differences were observed in the analyses above, I was interested in examining whether scores on any of the candidate mediator scales measured in the *initial* online session predicted pre-practice self-control performance (initial lab session).[9]

Stroop Task. Time 1 Stroop performance was regressed on the eight potential mediator variables listed above. There were no significant predictors of Stroop performance, although there was a non-significant trend for self-control instrumentality to predict performance, B = -0.012, SE = 0.008, t (98) = -1.423, p = .158. See Table 4.

Anagram Persistence and Performance. Time 1 anagram persistence was regressed on the eight potential mediator variables listed above; only self-control instrumentality emerged as a significant predictor: B = 87.06, SE = 39.70, t (99) = 2.193, p = .031. Likewise, self-control instrumentality was the only significant predictor of anagram performance: B = 2.35, SE = 1.15, t (99) = 2.047, p = .043. See Tables 5 and 6.

[9] Investigations of candidate mediator scales predicting self-control performance at Time 2, as well as *changes* in these scales predicting Time 2 self-control performance and *change* in self-control performance, are of course of strong interest and will be pursued in future analyses of this dataset.

Discussion

Self-Control Performance

In this study, I partially replicated past findings that practicing self-control can lead to improvement in self-control performance (Muraven, 2010a), using a larger sample than has been used in much prior work. Participants in the self-control practice (avoiding sweets) condition showed reduced interference on the Stroop task after two weeks of self-control practice relative to participants in the math control condition, but not relative to participants in the self-control diary control condition. This effect was present, though weaker, controlling for Time 1 Stroop performance, and there was no significant time x condition interaction; thus, there was not strong evidence of a *change* in Stroop performance from Time 1 to Time 2 for participants in the self-control practice condition. Furthermore, participants in this condition did not persist longer or perform better on a self-control task (anagram task) after depletion. The current study thus provides partial support for the idea that self-control practice may improve self-control performance, as assessed by the pre-depletion or "capacity" measure but not by the post-depletion or "endurance" measure.

Interpreting the Stroop Effects. One possible explanation for the pattern of Stroop results is that the effects were driven by differences in compliance across conditions. Perhaps all tasks led to improved self-control performance but did so less for participants in the math condition, who complied significantly less. While possible, I think this is unlikely the full story, given that compliance in the current study (~11 days out of 14) was high relative to studies in the self-control practice literature (on par with a mean of 11.68 days in Muraven (2010a)); even compliance in the math conditions (about 10 days) was high relative to other work (e.g., Sultan et al., 2012). However, when controlling for compliance in the model, the

main effect of condition did drop in significance (from $p = .034$ to $p = .080$). This suggests that it will be important to explore in future analyses how compliance with and engagement in the practice task may be an important factor in understanding self-control practice effects.

Notably, the Stroop performance of participants in the self-control diary condition did not significantly differ from either the avoiding sweets or math problems conditions. It is possible (and likely) that the self-control diary condition was more demanding than in previous work (Muraven, 2010a) given the modifications I made. Indeed, the self-control diary was rated as the most "demanding" condition overall. According to the strength model, relatively high or low difficulty should be irrelevant to self-control improvement, as long as completing the diary does not require overriding an impulse. If, however, difficulty or perceived difficulty is playing an important role (which we saw a hint of in Hui et al., 2009), it is possible that this helps to account for the current finding.

It is also possible that given the increased demands of the diary used in the current study, completing the diary did require overriding an impulse. It is interesting to note that in the original demonstration of self-control practice effects, a diary condition did serve as a self-control practice task; the authors noted that "the keeping of the diary was the exercise in self-regulation" (Muraven et al., 1999; p. 450). It becomes clear that designing appropriate control conditions that are equivalent in difficulty but do not require self-control is a challenging task. It could be argued (as in fact one participant pointed out) that completing the self-control diary in itself requires overriding the temptation to quit– not unlike my argument for why persisting on the anagram task is an appropriate measure of self-control. Future analyses will allow me to examine more directly if participants' daily experiences, as recorded in the self-control diary, relate in any systematic ways to self-control performance.

Interpreting the Anagram Effects. As discussed above, I did not find any evidence of a self-control practice effect on the post-depletion anagram task, my measure of self-control "endurance." Prior work has shown effects on both capacity and endurance measures, but there are not yet clear grounds for determining a priori which effect will emerge more strongly. A potentially significant limitation of the current study is that the anagram task administered at Time 2 may have been inadvertently easier than the anagram task administered at Time 1. For this reason, I analyzed anagram performance and persistence using Z-scores, but this limits my ability to most sensitively detect changes in performance by condition (unless there are rank-order changes in performance). In an on-going follow-up study, I am comparing mean persistence and performance in the two anagram tasks at baseline to assess whether the Time 2 anagram task was in fact easier or whether the effect on Time 2 raw scores is a real effect of time. If so, it could suggest that doing a demanding practice task for two weeks, regardless of whether it specifically requires overriding an impulse, improves self-control performance.[10]

Potential Mechanism of Self-Control Improvement: Value, Expectations, and Beliefs

In addition to improvements in self-control performance in the avoiding sweets condition relative to controls, I predicted that the improvements would be mediated by motivational changes (e.g., increased value of self-control, increased self-efficacy in the domain of self-control). For the key mechanisms analyzed in this paper, however, I found no evidence of the expected changes by condition. However, a few interesting patterns emerged when examining overall changes in these scales over time, and in examining their predictive power for self-control performance at Time 1.

[10] It could also be a learning effect or an effect of time, of course. If the follow-up study reveals no difference in performance between the two tasks at baseline, I will conduct a second follow-up study to rule out the alternative possibility that any improvement was due simply to the effect of time.

Changes Over Time. I found a main effect of time on some of the proposed mechanism scales, such that implicit theories about the ability to continue mental exertion increased post-practice (i.e. more unlimited theories); and self-control value, self-control instrumentality, and self-esteem marginally decreased post-practice. The change in implicit theories, though not stronger in the avoiding-sweets condition relative to controls, was in the expected direction. The decrements on the other measures, however, are not as readily explained. One explanation for the unexpected findings is that they were incidental effects: the post-practice measure occurred later in the academic term, when increased stress and distress from examinations are commonly high (Oaten & Cheng, 2006a; Job et al., 2010); thus, it is possible that participants were frustrated with self-regulatory efforts at the time and the effect was entirely due to external circumstances.

The observed reductions in self-control value and instrumentality may also have been a result of the study itself: regularly practicing a task you believe to be an exercise of self-control, regardless of condition, could have had a number of effects. For example, it is possible that in the initial online session, participants thought about items like "self-control pays off" (self-control value) and "self-control will improve my performance at school" (self-control instrumentality) more abstractly, or even with some degree of positive illusions, given their "implemental mindset" as they began the study (Taylor & Brown, 1994; Taylor & Gollwitzer, 1995). However, after two weeks of exposure to the practice task and increased awareness of self-control in general, they may have responded more soberly (or accurately). Another possibility is that participants did not feel that they performed well in their practice tasks and were not impressed, as I had hoped, by the notion that "such simple adjustments are possible and worthwhile." The reports of overall practice experience (means for "I cared about doing

my best on the practice tasks" and "I did well at the practice tasks" both approximate "neither agree nor disagree") and the marginal decrease in self-esteem are consistent with this interpretation.

Though more speculative, one further conjecture is worth noting: it seems possible that for many people, regular reflection on self-control performance, in itself, may actually hinder positive views of self and self-control. This is consistent with the marginal decreases in self-control value, instrumentality, and self-esteem discussed; and consistent, assuming the salience of self-control performance is strongest in the diary condition, with a number of other observations: (1) Though non-significant, the marginal decrease in each of these three variables was more pronounced in the diary condition; (2) The item "self-control will improve my self-esteem," found to be driving the decrease in perceived instrumentality, decreased only in the diary condition (trending interaction); (3) While self-efficacy for resisting temptations increased in the sweets condition, it decreased in the diary condition (marginal interaction); and (4) While implicit theories about the ability to continue resisting temptation increased in the sweets and math conditions, they decreased in the diary condition (significant interaction; also when including overall perceived performance as a covariate).

Still, this evidence should be taken with caution. In particular, it is important to consider the moderating role of the valence of perceptions of personal performance: reflecting on daily self-control successes versus failures may likely have different outcomes. The marginal decreases in perceived value and self-esteem could be an artifact of heightened salience of self-control failures and struggles (as it appears in my study), not reflection in itself. Fortunately, I will be able to better disentangle these issues in future planned analyses of the content of the self-control diaries; the diaries will provide a rich source of data on how these

perceptions may have shifted over time. I will investigate, for example, whether generally positive outcomes (higher ratings on "How successful were you?") or the perceived ease of attaining those outcomes (lower ratings on "How much did you work to resist temptation?") are predictive of overall perceived value at Time 2, or even of the measures of self-control performance.

Mechanisms of Self-Control Improvement. The initial set of analyses on potential mechanisms of self-control improvement was not promising. Self-control practice type was not related to increases in self-control value, instrumentality, or self-efficacy. This could mean, as the strength model would argue, that these proposed mechanisms are not the routes by which self-control practice has its effects. However, given that I did not strongly or fully replicate the prior work on the effects of self-control practice on self-control performance (e.g., Muraven et al., 1999, Muraven, 2010a, Muraven, 2010b) these null effects should be interpreted cautiously.

Further, as acknowledged earlier, the current analyses are by no means exhaustive. It is possible, for instance, that although self-control practice does not predict differences in the levels of these mechanisms at Time 2, it does predict changes in these mechanisms over time. These changes may be linked to self-control performance. It is also possible that self-control practice predicts changes in the *relationships* between these proposed mechanisms and self-control performance. For instance, perhaps practice does not change *how much* participants value self-control, but does change how the value of self-control is linked to self-control performance (e.g., becoming more tightly coupled). There are also a number of important measures not reported in the current analyses (e.g., working memory), as well as potentially significant moderators for the practice effect itself. For instance, it may be that self-control

practice is most effective for certain types of individuals (e.g., people low in conscientiousness) and that when I consider such factors, a different story about mechanisms will emerge.

There are also a number of ways that the current study could be extended. For one, other operationalizations of the same constructs might be explored that could perhaps better detect changes; many of the primary suggested mechanism scales were an initial formulation and not yet validated. It is even possible that many of the suggested mechanisms (e.g., affective evaluations of self-control and self-control self-efficacy) are not best measured through explicit endorsements at all. A number of studies have shown that goals can be initiated and completed entirely outside of conscious awareness (e.g., Bargh, Gollwitzer, Lee-Chai, Barndollar, & Trötschel, 2001); it is generally agreed that much of goal-related behavior occurs without awareness (Ferguson & Bargh, 2004; Ferguson, 2007). Finally, recall that for consistency with previous work (Muraven, 2010a), I explicitly told participants that the practice tasks should help build self-control: to determine generalizability, future work on mechanisms may benefit from creating a cover story that does not make self-control improvement salient.

Predicting Performance at Time 1. It is interesting to reflect on the analyses I conducted to examine the relation between initial measurement of these mechanism scales and self-control performance prior to the practice period. Self-control instrumentality, or the perceived perception of self-control as a means to important personal outcomes, emerged as the best predictor of self-control performance. This lends support for the motivational argument, especially given that these beliefs were a stronger predictor than trait self-control ability, an individual difference measure that is often related to self-control performance (Tangney et al., 2004; de Ridder, Lensvelt-Mulders, Finkenauer, Stok, & Baumeister, 2012).

Self-control instrumentality may, then, still be a strong candidate for a mechanism of self-control improvement.

One observation that appears consistent with this argument is the counter-effectiveness of a relatively difficult practice condition included in the original demonstration of self-control practice effects (Muraven et al., 1999). To test the range of self-control practice effects, Muraven and colleagues included a condition of overriding a dominant *emotional* response: participants were instructed to improve their emotional states as often as possible and keep a diary of their progress. They found that participants in this condition actually performed significantly worse in a post-depletion self-control handgrip task. One suggestion the authors made was that participants may have struggled unsuccessfully to improve their mood, relative to the more concrete and attainable goals of adjusting posture and keeping a diary. Repeated pairing of self-control attempts with a lack of success may have decreased perceived instrumentality and thereby undermined self-control performance. Given the potential importance of this mechanism, it may also be worth exploring other operationalizations of this concept. In a model of perceived control, Skinner, Chapman, and Baltes (1988) identify three independent beliefs that contribute to goal-directed behavior: agency (the perceived link from agent to means), means-ends (the perceived link from those means to outcomes), and control (the perceived link from agent to outcomes overall). The predictive power of my measure of instrumentality suggests the importance of means-ends beliefs for the effective exercise of self-control.

Implications and Future Directions

At this point, the current study provides more questions than answers about the extent to which self-control performance *can* be improved and *how* it operates. As discussed, the

performance effects were not fully consistent with the strength model and the potential mechanism effects were, therefore, more difficult to interpret. This work, however, did highlight key challenges for research on practice effects, such as controlling for difficulty, and identified promising areas for further study, such as limited resource beliefs and self-control instrumentality.

A trade-off of using a longitudinal design is reduced control: I did not attempt to directly manipulate my main predictors of interest. Thus, refining a new design for the kind of large-scale study described above may benefit from first probing hypotheses of the motivational model in the context of more controlled experimental designs. In one planned study, for example, I will build on previous research highlighting the independent contributions of the value of self-control performance (e.g., Muraven & Slessareva, 2003, Job et al., 2010) and expectations for self-control performance (e.g., Martijn et al., 2002, Clarkson et al., 2010) by directly manipulating both. This study will directly speak to one of the challenges faced by those arguing for a motivational approach to understanding self-control: strength model proponents argue that while motivational factors can restore performance in the short-term, limits to self-control performance will eventually be felt and observed in performance down the line (Muraven et al., 2006; Vohs, Baumeister, & Schmeichel, 2012). Thus, in a series of three demanding self-control tasks, participants will be randomly assigned to a high or low value condition (receiving a monetary incentive for performance or not) and a high or low expectations condition (feedback that a depleting task has or has not shown to be depleting in the past). I believe that participants will be able to persist across all three tasks, but only when both value and expectations are perceived as high. Similar kinds of manipulations could conceivably be applied to a more longitudinal study as well; for example, participants could be

given false information about whether practice tasks were or were not shown to help build self-control in past work.

Further exploration of the role of values, expectations, and beliefs, in both experimental and longitudinal designs, is critical for an understanding of how self-control operates. The potential that this line of research presents can only be realized if we know the mechanisms by which practice effects function and the situations to which they generalize. If, for example, means-ends beliefs play a strong role in the mechanism of self-control, it will be critical to design interventions that focus more on helping participants to build those associations, rather than interventions that focus on building ability per se. Ultimately, these applications have important implications that may help us to more effectively address the pervasive problems of self-control failure.

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Table 1

Scales Included in Online Questionnaire Sessions 1 and 2

Component	Source	Sample Item
Questionnaire 1 Only		
Regulatory Mode	Kruglanski et al., 2000	• "I am a critical person." (assessor score) • "I am a 'go-getter.'" (locomotor score)
Regulatory Focus & RF Composite	Higgins et al., 2001 Haws et al., 2009	• "Not being careful enough has gotten me into trouble at times." (prevention score- R) • "I see myself as someone who is primarily striving to reach my 'ideal self.'" (promotion score)
Big Five Inventory	John &Srivastava, 1999	(5 scores: extraversion, agreeableness, openness to experience, conscientiousness, neuroticism)
Behavior Identification Form	Vallacher & Wegner, 1987	• Asked whether they think of behaviors, like "making a list," as "getting organized or writing things down?"
Questionnaire 1 and 2		
New General Self-Efficacy Scale	Chen et al., 2001	• "I will be able to achieve most of the goals that I have set for myself."
Limited Resource Beliefs	Job et al., 2010	• "After strenuous mental activity, you feel energized for further challenging activities." (mental exertion belief score) • "It is particularly difficult to resist a temptation after resisting another temptation right before." (temptation belief score- R)
Locus of Control	Craig et al., 1984	• "A great deal of what happens to me is probably just a matter of chance." (R)
Trait Self-Control	Tangney et al., 2004	• "I am able to work effectively toward long-term goals."
Cognitive Appraisal Scale	Skinner & Brewer, 2002	• "I look forward to opportunities to fully test the limits of my skills and abilities." (challenge score) • "I worry about the kind of impression I make." (threat score)
Entity Theory of Personality	Chiu et al., 1997	• "People can do things differently, but the important parts of who they are can't really be changed."
Entity Theory of Intelligence	Chiu et al., 1997	• "To be honest, you can't really change how

		intelligent you are."
Self-esteem	Robins et al., 2001	• "I have high self-esteem."
Inclusion of Ideal Self in Self Scale	Adapted from Aron et al., 1992	• Asked which of seven increasingly overlapping circles best represents their relationship to their "ideal self"
Inclusion of Ought Self in Self Scale	Adapted from Aron et al., 1992	• Asked which of seven increasingly overlapping circles best represents their relationship to their "ought self"
Self-Control Value	Adapted from Tomaka et al., 1999	• "Exerting self-control is rewarding." • "I want to be able to exert self-control effectively."
Self-Control Efficacy	See Manstead & Eekelen, 1998	• "Whether or not I can resist temptations is completely up to me." • "There's a lot I can do to be sure that I stay focused on a goal over time."
Self-Control Instrumentality		• "Being successful at self-control will improve my reputation with family."
Working Memory Task	Hofmann et al., 2008	• (Asked to remember a series of solution values while evaluating the accuracy of simple arithmetic equations; 10 trials)

Note 1. Scales result in one score, unless otherwise noted; "R" signifies reverse coding.

Note 2. Scales in questionnaire 1 only were chronic backgrounds measures, assessed prior to the two-week practice period. Scales in questionnaire 1 and 2 (i.e., included both before and after the two-week practice period), were measures of potential mediators.

Table 2

Descriptive Statistics for Lab Measures By Time

Measure (Time 1)	Mean (SD)	Skewness	Kurtosis
1. Stroop interference score	0.0633 (0.0499)	1.106	2.368
2. Anagram persistence	319.14 (254.39)	2.124	5.043
3. Correct anagram solutions	14.00 (7.34)	1.189	1.698
Measure (Time 2)			
1. Stroop interference score	0.0492 (0.0369)	0.311	0.457
2. Anagram persistence	377.64 (245.48)	1.465	2.469
3. Correct anagram solutions	25.44 (10.62)	0.713	0.627

Table 3

Descriptive Statistics for Lab Measures at Time 2 By Condition

Avoid-Sweets Condition	Mean (SD)	Skewness	Kurtosis
1. Stroop interference score	0.0389 (0.0365)	-0.265	0.143
2. Anagram persistence	392.16 (233.73)	1.036	0.872
3. Correct anagram solutions	24.81 (11.45)	0.362	0.044
Math Condition			
1. Stroop interference score	0.0605 (0.0335)	0.296	-0.553
2. Anagram persistence	359.47 (268.49)	1.965	5.005
3. Correct anagram solutions	26.91 (10.51)	0.936	0.669
Self-Control Diary Condition			
1. Stroop interference score	0.04835 (0.0381)	1.017	1.358
2. Anagram persistence	380.69 (239.53)	1.393	1.679
3. Correct anagram solutions	24.65 (10.00)	1.091	1.968

Table 4

Predictors of Time 1 Stroop Performance

Scales	Time 1 Stroop Interference Score		
	B	SE (B)	β
Trait Self-Control	-0.006	0.010	-0.084
General Self-Efficacy	0.004	0.014	0.040
Unlimited Resource Beliefs (Mental)	0.002	0.006	0.032
Unlimited Resource Beliefs (Temptation)	0.002	0.007	0.035
Self-Control Value	-0.001	0.011	-0.006
Self-Control Efficacy	-0.002	0.012	-0.025
Self-Control Instrumentality	-0.012	0.008	-0.155
Self-esteem	-0.003	0.006	-0.451

Note 1. Higher Stroop interference scores indicate poorer inhibition.

Note 2. Full model F (8, 106) = 0.388, p = .925; R^2 = .031.

*$p < .05$

Table 5

Predictors of Time 1 Anagram Persistence

Scales	Time 1 Anagram Persistence		
	B	SE (B)	β
Trait Self-Control	48.620	51.547	0.127
General Self-Efficacy	7.373	70.769	0.016
Unlimited Resource Beliefs (Mental)	7.791	31.320	0.026
Unlimited Resource Beliefs (Temptation)	-0.894	33.281	-0.003
Self-Control Value	48.199	53.086	0.098
Self-Control Efficacy	10.071	57.275	0.025
Self-Control Instrumentality	87.063	39.695	0.230*
Self-esteem	-22.860	29.899	-0.101

Note 1. Full model F (8, 107) = 1.217, p = .297; R^2 = .299.

*p < .05; **p < .01

Table 6

Predictors of Time 1 Anagram Performance

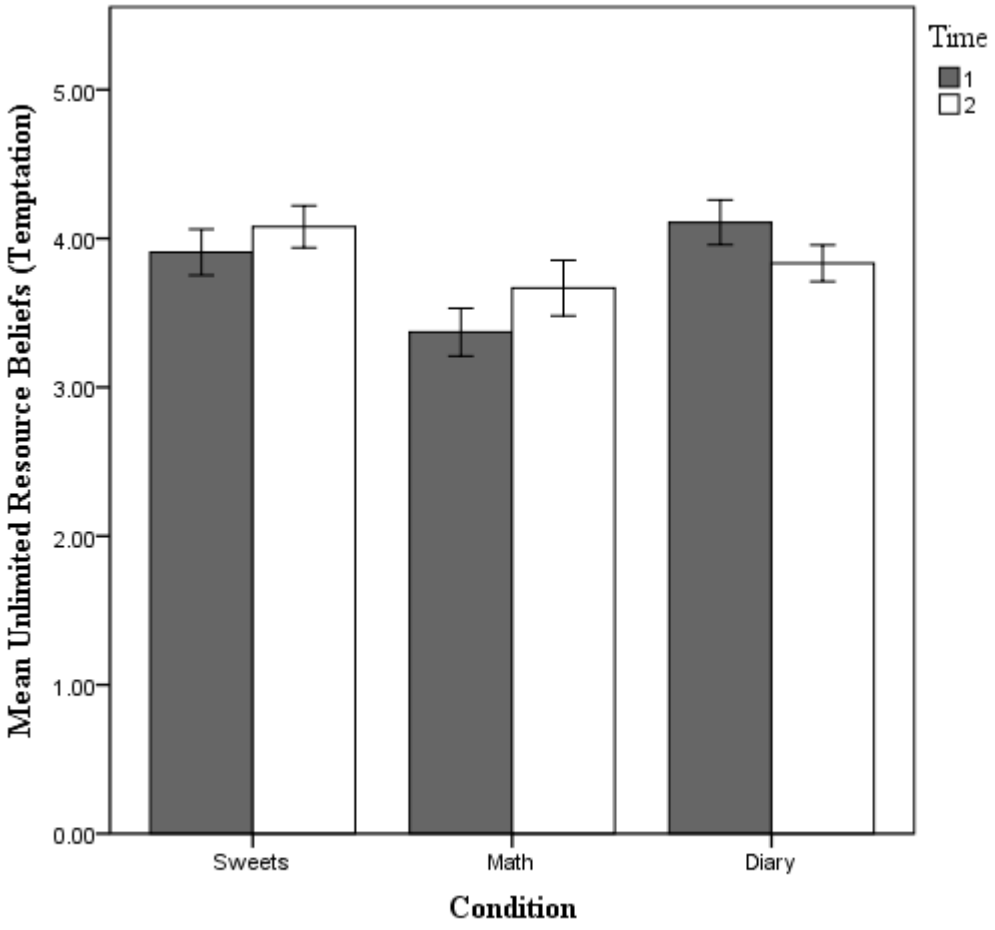
Scales	Time 1 Number of Correct Anagram Solutions		
	B	SE (B)	β
Trait Self-Control	1.023	1.488	0.092
General Self-Efficacy	-1.632	2.043	-0.120
Unlimited Resource Beliefs (Mental)	-0.996	0.904	-0.117
Unlimited Resource Beliefs (Temptation)	0.643	0.961	0.086
Self-Control Value	-0.346	1.533	-0.024
Self-Control Efficacy	-0.433	1.654	-0.038
Self-Control Instrumentality	2.346	1.146	0.215*
Self-esteem	-0.874	0.863	-0.133

Note 1. Full model F (8, 106) = 0.388, p = .925; R^2 = .031.

*p < .05; **p < .01

Figure 1

Change in Unlimited Resource Beliefs By Condition



Note 1. Error Bars: +/- 1 SE.